in the basalt-lava, so as to give trustworthy indications of the real movements of the ground. Perturbing causes which would have kept the pendulum in agitation, such as wind, the passing of people, carriages, &c., had not on that day to be taken into account.

I think it rather improbable that secondary and subsequent shocks, coming from the same centre as the first one, were the cause of the observed fact: a much more probable explanation would be that the whole hill on which the Observatory is built maintained during the whole time the particular state of trembling produced by the first shock.

Rocca di Papa, Rome, March 18. EMIL ODDONE.

Striated Surface under the Cromer Drift.

At the beginning of the present month (March) there were some good Forest Bed exposures in the neighbourhood of Cromer. Somewhat less than a quarter of a mile south-east of East Runton, there was an exposure I had not noticed before. It consisted of a smooth, hard surface of ferruginous sand, not unlike some of our Trias beds, except that below it became softer. This slab of sandstone projected six yards obliquely from under the cliff, or rather talus in front of the cliff, with a slight dip to the west inwards, its outward face rising one foot above the sand of the foreshore. Upon the outer or longer exposed part there was no indication of striæ, but toward the inner or more recently exposed portion faint grooves could be distinguished, becoming more distinct the nearer it approached the cliff; two feet from the cliff they were distinct and numerous. The main direction of striæ ran due north and south, a few crossed from the north-north-west and north by east, but none deviated from the general direction beyond these points. They extended entirely across this part of the exposure, viz. 4 feet, and continued as far as the surface could be uncovered. Resting immediately above, as far as could be made out among the recent talus, was the highly-contorted drift sand and loam usually observed hereabouts; no flint nodules, stones, or boulders could be seen in them, and it is difficult to understand how these, if they had been present, could groove the bed so persistently in one direction when shoots took place.

About half a mile to the south-east and one mile to the north-west, the cliff sections this year show the soft sand and loam beds bent into anticlines, thrust back to a vertical position like a pack of cards on end, their central axes pointing to the north. This form indicates that the force came from the same direction as the mean striæ engraved upon the underlying surface. Three points of interest will be observed:—

(1) That the ice which caused the striations could hardly have contorted the beds above them; it probably shrank back and allowed the drift to be deposited, and then encroached once more and contorted the overlying beds.

(2) Soft beds of sand and loam could not be twisted or contorted into ribbon forms unless a considerable load lay above to prevent them breaking up and becoming disarranged.

(3) We should hardly expect to find striæ preserved upon a comparatively soft surface under what looks like bedded sand or loam; but unfortunately this point cannot be clearly established in consequence of talus obscuring the bed immediately above; but, bearing upon this point, it may be interesting to state during the summer of 1890 I found at Penrhyn, Nevin, Carnarvon Bay, a highly glaciated and striated rock surface which had been recently exposed. This *roche moutonnée* was overlain by a drift cliff of bedded sands and loams 100 feet high, resembling the Cromer drift in every respect except the contortions; these striations were from the east-north-east or seaward side also. WILLIAM SHERWOOD.

Sutton Coldfield.

Pilchards.

In this far-off corner of the world the news has only just reached me that my name has been quoted in your valuable columns with Mr. Cunningham's article on the growth of the pilchard or sardine. As it is a matter which much interests me, I should like to have a word or two on the subject. Personally, I have no doubt as to the identity of the pilchard and sardine. Seeing the matter has been so well threshed out by our greatest ichthyologists—Couch, Day, and Günther—the spawning of the fish being only a question of local conditions, and not even giving us aid in determining the species—note the doings of the herring when about this work around the islands of Great Britain, which keeps shedding its roe for eleven months out of the twelve in these waters. As to the question of the English pilchard being so much larger than those of other countries, this to my mind is a subject of *grave doubt*, and I fear Mr. Cunningham's informants have not looked up the matter thoroughly.

When I was a juror in the London International Exhibition in 1883, in the Spanish department we gave awards to exhibitors who had pilchards of the same size and cure as those of Cornwall. Again, the principal markets for our Cornish pilchards are those of Italy. But in this business we are not alone, for side by side with our fish are the Spanish pilchards, cured like our own; generally they are a little smaller than ours, answering to our summer fish.

But in this past season they are decidedly our rivals, and in such quantities as to bring the price down in such a manner as to leave us with scarcely a margin of profit through their being just equal in size to our Cornish output.

In the past season we calculate that Cornwall and Devon have sent out about 25,000 casks of pilchards into Italy; but we should not be surprised to find that Spain has sent forwards into the same country over 30,000 casks.

We began the season by sending our fish forwards in September, and were rather surprised to find the Spanish merchants had glutted the Italian markets with fish in August, and the imports went on till near Christmas, which seems to indicate they were catching pilchards on the coasts of Spain in the summer and autumn of the year. When this question of the size of the pilchard has been fairly looked up, I think it will be found the size of the fish of any coasts will be chiefly governed by the facts that abrupt and exposed coasts, having a heavy sea and strong tides along them, will have the strongest and largest fish swimming in its waters; while the bays, and narrow and protected seas and inlets, will have the young, weak, and smaller fish in them, the nature of the foods having the lesser influence.

I have been led to believe the Bay of Douarnenez is the deepest bay on the coast of Brittany, and in it is carried on the largest fishery for young pilchards on the French coasts; while off the more abrupt parts of the coast of Spain the pilchards are large, like those on the exposed parts of Cornwall. And no doubt if the French coasts were well looked up, the same facts would come out respecting the fish there.

Fortunately for the French and Spanish fishermen, up to date they have not interfered with the food of the young pilchard; or, if they have, evidently they have substituted another in its room, viz. *cods' roe;* hence they have these little ones in their bays still; but we have driven out ours by starvation, as will be seen further on.

I was rather surprised to read Mr. Cunningham's statement when he said I had told him that I had never seen pilchards in Cornwall of the same size as the French sardines, for really I have seen millions just like them for size.

Possibly the error came from my misunderstanding his question, as we cannot see or get them now because of our altered conditions.

If Mr. Cunningham will consult F. Buckland's familiar history of British fishes (p. 109), he will see a letter from me confirming my statements, and written in the year 1872. In the year 1884 I received the medal of the Falmouth Polytechnic Society for exhibiting a series of small pilchards, showing their growth and ages in six stages—the smallest being less than an inch in length—up to the two years old full-grown pilchard.

Before the railway ran into our county, our bays were full of these little fishes in the summer months; and when our seines inclosed pilchards, the first question was their size, as pilchards under eight inches in length were useless for exporting purposes; consequently small fish were quickly turned back into the sea alive.

But sometimes, in the excitement and darkness of the night, the men were mistaken in the size of the fish, and took them into their boats; and when the daylight undeceived them the fish had to be carted off for manure. I have often seen them on our piers, piled up four feet high and hundreds of feet in length, waiting the waggons.

But all this has passed away, and we have not one pilchard seine left here to inclose pilchards should they again visit us. But their coming is very improbable, as the quantity of food necessary for their sustenance is so much diminished that if the

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old numbers came on the coasts they would probably die in our waters for want of food. And this sustenance was evidently one of the greatest delicacy. Full-grown pilchards have been known to feed up to yielding from three to seven gallons of oil to the hogshead of 3000 fish when having their fill of it. Their food was young Crustacea, and evidently was the larval forms of some crab or crabs which live on our coasts.

I think a few words will make this plain; in considering the great crab—*Cancer pagurus*—in the sea, the sexes stand in relation to each other of about one male to eight or ten females, the latter spawning from one to two million eggs. These, when hatched out, pass through several distinct larval changes in the surface of the sea before dropping down on the sea bottom.

Creatures having such vast procreative powers, when all the conditions of life are favourable, must produce more young than are wanted to make up for the wear and tear of the race; hence in our first outlook we seem in danger of having a plethora of crabs. But Nature, true to herself, has a police force at hand to prevent overcrowding. This is found in the pilchard, who attacks the crabs in the surface of the sea when in their zoe forms; while at the sea bottom, if they are yet too plentiful, those powerful skates (*Raia batis* and *Raia lintea*), with their long, sharp, hard noses, make their appearance among them, routing them out of their hiding-places among the rocks, and with their powerful jaws and teeth making short work with these crabs.

Hence, in the olden times, when there was no demand for the female crab, even at sixpence per dozen, and when they lay off our coasts in millions, and again throwing off their countless millions of eggs, there was seldom any lack of either large or small pilchards in our bays in the summer months of the year.

But since the extension of the railway systems throughout our land, and the demand came for all the crabs, not only have the large pilchards been scarcer, but they have so fallen off in condition as not to yield above one gallon and a half of oil to the hogshead, and the French sardine-sized fish has disappeared altogether.

It is certainly very satisfactory at this date to know that Mr. Cunningham has found them in their new haunts further out at sea; and that he has also verified the facts of the size and the ages of the pilchards given in my exhibits at the Falmouth Polytechnic so long ago. MATTHIAS DUNN.

Mevagissey, Cornwall, March 22.

On the Boltzmann-Maxwell Law of Partition of Kinetic Energy.

IN the very valuable Report on Thermodynamics drawn up for Section A of the British Association by Messrs. Bryan and Larmor, and now recently published, there is a remark upon the Boltzmann-Maxwell law of partition of Kinetic Energy, upon which I should like to be allowed to make a few comments. The Report says, in fact, after noticing the attempts to extend the theorem from the case, originally contemplated by Boltzmann, of molecules composed of discrete atoms under mutual forces, to the general case of dynamical systems determined by generalized co-ordinates: *It has now been proved beyond doubt that the theorem is not valid in this general form;* and quotes as a test case a paper by Prof. Burnside to the Royal Society of Edinburgh, on the collisions of elastic spheres, in which the centre of mass is at a small distance, c, from the centre of figure. In this paper, doubtless, results are arrived at, after a vigorous and able treatment, inconsistent with the law now under consideration; but there is, I think, an oversight, pointed out by Mr. Burbury in a paper recently read to the Royal Society of London, which vitiates these conclusions and leaves the matter where it was before.

Prof. Burnside, in fact, has omitted to introduce the frequency factor of collisions in proceeding to take his average, so that, whether his result be correct or not, for the average of all *possible* collisions, it is not correct for the average of all collisions *per unit of time*, and it is this last which is important for the test of permanence of distribution.

When this frequency-factor is introduced and the approximation carried, as in Prof. Burnside's paper, to the second power of c, it will be found, I believe, that $\frac{A}{k_1} = \frac{1}{h}$ and not $\frac{2}{h}$, so that if this statement is correct, we are hereby furnished with a confirmation of the Boltzmann-Maxwell law by an independent treatment.

The process is somewhat intricate, and too long for insertion here.

I should like to make a few additional remarks on a view expressed by Prof. Burnside, which is doubtless widely, but I think not quite reasonably, shared by many eminent mathematicians, to whom this theorem of partition of Kinetic Energy is a stumbling-block.

He says, in the paper referred to, "The method of proof adopted by Watson, following Boltzmann, is so vague as to defy criticism or attempts at verification," but I really think the vagueness consists in the generality of the conclusion and not in the method of proof. To establish a proposition applicable to all conceivable cases of collision, either in a field of no force, or of forces of any kind, requires a method of proof which, whether true or false, must of necessity be as general, or, if you please, as vague, as the conclusion; but, in point of fact, Boltzmann's method adapts itself readily to every case which, like this of Prof. Burnside's, admits of practical treatment. For example, in this very case of the colliding spheres with centre of mass distance (c) from that of figure, Boltzmann's method would assume that the number of spheres with lines of centres in any direction, and with component velocities of translation of C.G. and of angular velocities round the principal axes lying between u, $u + du$, &c., &c., ω_3, $\omega_3 + d\omega_3$, was

$$\phi(u, v, w, \omega_1, \omega_2, \omega_3)\, du \ . \ . \ . \ d\omega_3.$$

Suppose, then, the circumstances of the two spheres to be distinguished, as in Prof. Burnside's notation, by the great and small letters, U, u, &c., Ω, ω, &c., and let the corresponding dashed letters denote these respective quantities after collision.

Then, as proved in Prof. Burnside's paper, we have—

$$U' = \frac{2u + c^2(K+k)U - 2c\varpi}{2 + c^2(K+k)}, \quad u' = \frac{2U + c^2(K+k)u + 2c\varpi}{2 + c^2(K+k)};$$

$$\Omega'_1 = \Omega_1 + \frac{2cP}{A} \cdot \frac{U - u + c\varpi}{2 + c^2(K+k)}, \quad \omega'_1 = \omega_1 + \frac{2cp}{A} \cdot \frac{U - u - c\varpi}{2 + c^2(K+k)};$$

$$\Omega'_2 = \Omega_2 + \frac{2cQ}{B} \cdot \frac{U - u + c\varpi}{2 + c^2(K+k)}, \quad \omega'_2 = \omega_2 + \frac{2cq}{B} \cdot \frac{U - u - c\varpi}{2 + c^2(K+k)};$$

$$\Omega'_3 = \Omega_3 + \frac{2cR}{C} \cdot \frac{U - u + c\varpi}{2 + c^2(K+k)}, \quad \omega'_3 = \omega_3 + \frac{2cr}{C} \cdot \frac{U - u - c\varpi}{2 + c^2(K+k)};$$

where A, B, C are principal moments of inertia through C.G.; P, Q, R, p, q, r are quantities depending on the relative situations of the principal axes, the line joining the centres of figure and mass, and the line of centres at collision, and not affected by that collision.

$$\varpi = P\Omega_1 + Q\Omega_2 + R\Omega_3 + p\omega_1 + q\omega_2 + r\omega_3,$$

$$K = \frac{P^2}{A} + \frac{Q^2}{B} + \frac{R^2}{C}, \quad k = \frac{p^2}{A} + \frac{q^2}{B} + \frac{r^2}{C},$$

and the velocity of approach therefore equals $U - u + c\varpi$.

The Boltzmann method, therefore, would require, for the permanent or special state, the condition that

$$\phi(u \ . \ . \ \omega_3)\phi(U \ . \ . \ . \ \Omega_3)\, du \ . \ . \ . \ d\Omega_3\, (U - u + c\varpi)$$

should be equal to

$$\phi(u' \ . \ . \ . \ \omega'_3)\phi(U' \ . \ . \ . \ \Omega'_3)\, du' \ . \ . \ . \ d\Omega'_3\, (U' - u' + c\varpi'),$$

because, when this condition is satisfied, and only then, can the average number of spheres with velocity components in the undashed state and line of centres parallel to the x axis (which may be any direction), be equal before and after collision, inasmuch as those in the dashed state with velocities reversed enter into the undashed state.

In determining the multiple differential $du' \ . \ . \ . \ d\Omega'_3$, we may neglect the consideration of the resolved velocities in the tangent plane, v, w, V, W, inasmuch as they are unaltered at impact, and we have to evaluate the quantity—

$$\frac{1}{\{2 + c^2(K+k)\}^8} \cdot \begin{vmatrix} c^2(K+k), & 2, & 2cP, & 2cQ, & 2cR, & 2cp, & 2cq, & 2cr \\ 2 & c^2(K+k), & -2cP, & -2cQ, & -2cR, & -2cp, & -2cq, & -2cr \\ \frac{2cP}{A}, & -\frac{2cP}{A}, & 2 + c^2(K+k) + \frac{2c^2P^2}{A}, & -\frac{2c^2PQ}{A}, & -\frac{2c^2PR}{A}, & -\frac{2c^2Pp}{A}, & -\frac{2c^2Pq}{A}, & -\frac{2c^2Pr}{A}, \end{vmatrix}$$

and so on for eight lines.

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